

When Being a Mechanist Wasn't So Bad: Reply to Moxley

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Moxley (1996) objects to my characterization of Loeb and his influence on Skinner and behavior analysis. As he correctly points out, Loeb's views underwent a significant change in the later stages of his career. I agree that accounting for this shift would be an interesting and important topic in its own right. (This was one of many topics that received detailed coverage in Pauly's 1987 biography of Loeb.) But that was not the focus of my paper. Rather, the focus of my paper (as pointed out in several places) was on Loeb's earlier career, specifically the years 1890 to 1915. These were the peak years of what Pauly called "the engineering ideal" in biology, championed by Loeb. These were the years that exemplified Loeb's open-ended approach to biological problems founded on prediction and control, and the years most relevant to Skinner and behavior analysis. It was the Loeb of these years to whom Skinner acknowledged an intellectual debt in several of his autobiographical writings.

Reconciling my view with Moxley's may therefore be as simple as pointing out that we were focused on different parts of Loeb's career—what one might call early Loeb versus late Loeb (as one might distinguish early from later Wittgenstein). But even if we were to restrict our focus to early (pre-1915) Loeb, Moxley's view still appears to be very different from mine. The differences appear to center around current and historical usages of the terms *mechanist* and *mechanistic*. Moxley presents the received view of

Loeb, as "archetypal mechanist," rigidly committed to a type of explanation that seeks to reduce all phenomena to elemental building blocks. According to this view, Loeb joins a more or less continuous line of mechanistic thinkers dating from 17th century mathematicians and philosophers and extending through to the present. Loeb enters into this mechanistic tradition (and into the history of psychology) via his work on tropisms, closely related to reflexes, stimulus–response associations, and the like. From this perspective, Loeb's influence on Skinner and behavior analysis was peripheral, roughly comparable in importance to other well-intentioned but misguided associationists of that period.

My paper suggested an alternative view of Loeb's participation in the history of behavior analysis, one that focused more on the methods utilized by Loeb and Skinner and less on present-day metaphors. I deliberately attempted to avoid the terms *mechanist* and *mechanistic*. I did so for several reasons. First, such terms have come to be used in so many different ways that they are easily misunderstood. A recent interchange in this journal (Spring, 1993) illustrates the variety of ways in which the term *mechanistic* has come to be used by behavior analysts. The meanings of the term multiply even further when we consider its use in other sciences such as biology (Loeb's discipline) and physics (the prototypical mechanistic science), not to mention its varied usage in the vernacular. The meaning of *mechanistic* thus varies widely from context to context, an unsatisfying state of affairs for a science such as behavior analysis that prides itself on precise terminology.

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Second, I believe terms like *mechanistic* promote “Whiggish” or presentist interpretations of history—the tendency to tailor past facts to fit present-day proclivities and predilections. Whatever these terms might mean today, clearly they meant something quite different in Loeb’s day. To call oneself a mechanist in Loeb’s day was to call oneself a scientist (Morris, 1993). In advocating a mechanistic approach to the life sciences, Loeb was standing up for determinism and reason against metaphysics and romanticism, which were creeping into the natural sciences at the time. The term *mechanistic* in Loeb’s day was an honorific term, not the much-vilified term it has become in some present-day circles. To label Loeb a mechanist according to current usage is to remove the term from its historical context and to trivialize a very complex position. Loeb was a mechanist when that was still an honorable thing to be.

Third, terms like *mechanistic* encourage us to think in terms of misleading and oversimplified dichotomies. Thus, one is either a mechanist or a contextualist, a determinist or a selectionist, a reductionist or a holist, guided either by logic or by effective action. As Marr (1996) has pointed out in a recent essay, however, such dichotomies oversimplify what are actually very complex issues, and grossly misrepresent the facts to be accounted for by a natural science. Nature doesn’t fracture along such tidy lines, so why should our descriptions of it? To take just one example from Moxley’s paper, consider the distinction between random variation and determinism. Does it really have to be one or the other? Can’t one accept both random variation, as providing the raw material for evolutionary change, and selection, as a deterministic agent of change? If the retreat from mechanistic thinking also

requires an abandonment of deterministic principles, then, like Loeb, I am happy to call myself a mechanist.

Ultimately such labels, however, are probably of little use because they fail to capture what scientists actually do. When we isolate controlling variables, are we not, in a sense, dissecting some part of the world into its constituent parts? Is this not how we go about identifying the “natural lines of fracture” (Skinner, 1935, p. 40)? This sounds like the reductive-analytic path of mechanism. But isolating such variables gives one practical control over behavior; that is, it meets with effective action. This sounds like the pragmatic truth criterion of contextualism. So, when we engage in scientific activity, are we mechanists or are we contextualists? We, like Loeb and Skinner, are probably a little of each. The isolation of controlling variables and the practical consequences that result from such control are flip sides of a coin—two ways of looking at what scientists do. To hold one superior to the other is to confuse two aspects of scientific activity for two separate activities. In my paper I suggested broadening our view of prediction and control in a way that incorporates both analysis and effective action, making it unnecessary to draw a distinction between them or between the different worldviews they presuppose.

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